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FURTHER NOTE ON THE ERUPTION OF VESUVIUS IN 79 A.D.

IN an article published in this JOURNAL in 1918 (Vol. XXII, No. 3), dealing with certain phaenomena exhibited by Vesuvius in the famous eruption of 79 A.D., I raised again the question whether the Italian volcano might not have belched out against Pompeii such a fiery blast as that from Mont Pelée which in 1902 destroyed the city of St. Pierre in Martinique. This suggestion I had first made long ago in my notes on the sixteenth and twentieth letters of Pliny's sixth book, published in 1903 in my edition of selections from that author's correspondence (London, Macmillan and Co.). The notes concerned were written soon after the great eruption of Pelée, and were actually in print, though not yet issued, before the observations of Professor Angelo Heilprin on the eruption appeared.

Recently this repeated suggestion of mine has been mentioned with some courteous demur in an article on 'The Past Decade of Pompeian Studies' in the *Classical Journal* for April, 1920 (Vol. XV, No. 7) by Mr. A. W. Van Buren, of the American Academy in Rome. His objections are such as might naturally occur to a scholar especially versed, as he is known to be, in Pompeian archaeology. The subject concerned is of so perennial and dramatic interest, both scientifically and historically, that I shall probably be pardoned for some comment on the objections, though I should like to disclaim any distinctly partisan attitude on what is possibly an insoluble problem. Yet progress toward a solution can only be made through discussion and the interchange of criticism.

In the first place Mr. Van Buren remarks that "if there was a hot blast it came not as one would have expected at the beginning but at an advanced stage of the eruption." It is quite true that, if it came at all, it came not at the beginning but at the time of the recrudescence of the eruption and at its most violent point. This increase of violence is plainly marked in Pliny's description, and occurred after a depth of *lapilli* to the measure of several feet must have already fallen on the doomed city, and when thereby the lower portions of the walls of the buildings were protected and braced against any violent horizontal impact through the air. Most of the inhabitants had already fled beyond the gates. I do not know why Mr. Van Buren thinks "one would have expected" otherwise, unless he has not familiarized himself with the accounts of the Martinique eruption. That is the only instance of a catastrophe of this character thoroughly known to modern observers. On it alone can be founded any judgment about what one might expect in a similar case, unless we may also draw safe conclusions from the less familiar eruption of the Japanese volcano Bandaisan, in 1888, which seems to have been of like character. In the Martinique eruption the sequence of events corresponded in all essential aspects to that postulated by my hypothesis at Pompeii. Mont Pelée after an almost unbroken quiescence of unrecorded centuries had now been in lively eruption for some days; the phaenomena were increasing in violence; the surrounding country, including St. Pierre itself, had already been deluged with ashes; then came the terrific blast, followed twelve days later by yet another of similar intensity that completed the devastation. The like fiery tornado that destroyed Morne Rouge occurred after a still further interval of more than three months, during which time the mountain had never ceased to exhibit volcanic action of varying character and intensity. Certainly, then, there is no reason for the presumption that such a fiery blast at Pompeii must have come, if at all, at the beginning of the eruption of Vesuvius. The justified presumption lies quite in the contrary direction, and is not in the slightest degree repugnant to my suggestion, but on the other hand quite consonant with it. Of course this is not to say that such a hot blast could not issue at the initial stage of an explosive volcanic eruption. It is only to say that at Martinque it did not, and that in the nature of things there is no reason why it should mark the first outbreak, when, indeed, its violence might be chiefly expended in rending the rock-masses opposing its free exit. Both observation and theory are fatal to Mr. Van Buren's presumption.

My critic further objects (for it is apparently meant as an objection to the notion of a hot blast at Pompeii) that "its effects were very unevenly distributed." The sentences that immediately follow in his review appear to contain the specifications that he has in mind under this general head. I could not have arrived at them from conjecture, for the effects of the blast would have appeared to me to be in all but one particular very evenly distributed, so far as we are now able to discern them. But Mr. Van Buren's specifications are, (1) that most of the inhabitants of Pompeii evidently had time to escape without the walls, and had actually thus escaped; (2) that those who were finally overwhelmed within the city "had lived, as a rule, through the shower of pumice-stones (*lapilli*) and into the rain of volcanic dust (*cineres*)"; (3) that the plastered walls discolored possibly by heat may have been thus affected by local conflagrations; and (4) that such substances as hemp, wood, and lead have been found in such condition as would not suggest that they have been subjected to the action of intense heat.

It is evident that of these four points, which are all indubitable as to fact, the first two are of force against my suggestion only on the presumption that the hot blast must have come, if at all, at the beginning of the eruption. On that presumption these two objections would, to be sure, be damning. The hot blast could not have had the other effects noted and yet have spared human life almost entirely. But I have pointed out that the underlying presumption about the time of the blast is itself unjustified. The objections founded upon it therefore fall to the ground.

With regard to the third of Mr. Van Buren's objections it may be remarked that the utmost extent to which the postulation, if otherwise reasonable, of a uniform local-conflagration cause could go in rebuttal would be to indicate that it is unnecessary to suppose a hot blast in order to account for the discoloration of yellow wall-pigments, the uneven distribution of which phaenomenon could more simply and with greater satisfaction of the principle of logical uniformity be assigned to the agency of local conflagrations. It cannot be asserted that to postulate a hot blast as the cause of the discolorations is to propose a less simple and probable cause in place of one more simple and probable. Had it not been for the disaster at Martinique, it would, to be sure, have appeared most wildly improbable, and even impossible, that such a blast could have swept Pompeii: since the event of 1902 it should not appear at all improbable *per se*, but quite possible. Yet one's judgment of the degree of probability of the actual occurrence will of course depend on the conclusions to be drawn from the entire range of observed phaenomena, and on the consideration whether any other single hypothesis, or combination of hypotheses, will equally well or better explain all the facts of present observation or past record. Certainly not all of them have been satisfactorily explained by the varying methods hitherto favored.

It is not entirely satisfactory to suggest merely that the discolorations may have been due to local conflagrations. In order to be most effectively in point on the present issue it would be necessary to prove, (*a*) that in all the cases where the discoloration has been observed there were actually such local conflagrations; (*b*) that the discoloration could not equally as well have been caused by a hot blast; (*c*) that the local conflagrations could not have been themselves started by such a blast. I note with interest that Mr. Van Buren apparently does not hold the late Professor Mau's belief that the discoloration is due to some mysterious slow chemical action. It certainly is inexplicable on this theory to find the oxydization working in some houses and not in all. Of the apparently selective action in this particular of a postulated hot blast I shall speak further on.

Now it may be that in those houses with marked change of yellow-ochre wall-pigments to red there are also found other indubitable signs of local conflagrations that are not to be found elsewhere. But I do not know of satisfactory evidence to this effect. I could not get it from printed records of the excavations nor from my own observation. I would by no means assert that it does not exist, though Professor Mau apparently did not think it exists, for he accounted for the discolorations otherwise. But it will be noted that in order to be satisfactory the evidence must be independent of the present state of the walls themselves (the cause of which condition is the matter at issue), and it must be such as does not exist in the case of buildings with no such discolored walls, unless indeed there can be shown sufficient reason for the variation of appearance under similar conditions. Until such evidence is forthcoming it is no argument against belief in the possible occurrence of the suggested hot blast to point out that the observed discolorations might have been caused by local conflagrations, especially if it cannot be proved that these were not themselves caused by a hot blast.

The only strong objection to the theory of a superheated tornado as the cause of the discolorations might be that only in occasional houses has such discoloration been noted in any extensive measure, while there might be no apparent reason why the postulated hot blast should not have reddened the yellow-ochre pigment in all houses thus decorated, if it did so in any. To this it is reasonable to say that the walls would be likely to show marked discoloration only in cases where the accumulated *lapilli* had left them still exposed, on account of some local circumstance now perhaps untraceable. I should, however, like to examine again the extant reddened walls with this point in mind. It would also be interesting to make the actual test whether pieces of Pompeian yellow walls, if exposed for an instant to extremely high temperature, or for a longer time to the heat of a bonfire in close proximity to them, would undergo the reddening change of color. But the specimens from Pompeian walls that I possess are too valuable to be used as the *corpus uile* of such experimentation. I may try it on blocks of plastering colored for the purpose with yellow-ochre pigment. It should be noted that such changes of yellow wall-coloring to red are definitely reported from St. Pierre, where, however, I cannot be certain that the change was due to the hot blast rather than to the resulting conflagrations. There may also have been at Pompeii mere freakishness in the action of a hot blast, as I have mentioned below. And I could, if necessary, surrender to the local-conflagration theory (which Mau held as a fact, but rejected as the cause of the color-changes) all the cases of discoloration, without finding it necessary to withdraw the suggestion of the hot blast to account for possibly the starting of the local conflagrations themselves, and certainly for many other—perhaps for all the other—observed phaenomena that are pertinent to the case.

On the fourth point—that of the occasional preservation of hemp, wood, lead, and like destructible or fusible substances—I may remark that it has been my strong impression that as a rule, if not invariably, where substances like these have survived in good condition, it has been either under the especial shelter of masonry, or at the lower level, where the deep stratum of already fallen *lapilli* would offer protection against the momentary effects of great heat. I still retain that impression. But even if it is wrong, the cases of the preservation of exposed substances of the sort mentioned must be relatively very few, so few that they might be reckoned among the unaccountable vagaries of the fiery tempest. Such freaks of partiality in conservation occurred in considerable numbers at St. Pierre, and attracted the attention of various observers. Some of the scorched bodies, for example, were found stripped of their clothing (possibly, to be sure, not by fire but by the mechanical force of the blast), while other bodies were terribly burned under their clothing, while the clothing itself was uninjured. Doubtless in many instances, if not in every instance, of such apparent freakishness in treatment it might have been possible at the time to determine a sufficient reason for the discrimination from the circumstances of relative exposure. The same investigation might well be possible at Pompeii, in so far as there were instances there of similar difference of effect upon readily destructible objects above the level of the *lapilli*. A general comment of Professor Heilprin is perhaps worth quoting: "A scorch-blast that clears all human life before it, and leaves in places untouched objects that are normally thought to be most destructible, has many things for its characteristics which science has still to learn" (*Mont Pelée and the Tragedy of Martinique*, p. 120).

These brief paragraphs must serve as answer to the observations of necessity so briefly put forth in Mr. Van Buren's review. To me they appear to be sufficient answer. And I note that my friendly opponent (if he is really an opponent, and not merely a naturally and properly hesitant comrade) said nothing about a number of other points in my former article that appeared to me of greater importance in their bearing on the main question. I must, by the way, object to his pure assumption that the total wrecking of the exposed portions of most structures that projected above the lower strata of ejecta (those of *lapilli*) was due to the earthquakes mentioned by Pliny. It may have been so. But, to judge from St. Pierre, a blast of the Martinique intensity would have done the business quite as effectively—indeed, I am inclined to think even more effectively. For the earthquake must have worked mainly through the ground. It could hardly, I should think, have been effectively propagated through the loose blanket of freshly deposited and uncompacted bits of light pumice (the *lapilli*). Yet the only parts of walls that were shattered into fragments were those that projected above the *lapilli*. The portions of walls below were practically uninjured. If the earthquake caused the wreck, could the light and loosely lying pumice so brace the parts of the walls against which it was piled as to defend them perfectly from the stress of a force working in a horizontal back-and-forth movement (cf. Plin. *Ep.* VI, 16, 15) communicated chiefly through their solid foundations? Could this apparently slight support be responsible for such a marked difference between the fates of the lower and of the higher portions of the walls? On the other hand, the postulated blast would readily shear away uncovered portions too suddenly to permit the force to be communicated to the already buried parts.

I would also reëmphasize my remark on the small amount of wood preserved in the ruins—unexpectedly small in amount and curious in its charred aspect, if we are to believe that there was no superheated blast, and that there were only isolated and occasional conflagrations, none of them involving any considerable areas.

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